

September 3, 1955

Dr. John von Neumann
Atomic Energy Commission
Washington 25, D.C.

Dear Dr. von Neumann:

Thank you for your letter of August 15. I was away from Madison at that time and have taken the first opportunity to reply.

As a result of this discussion, and others with Szilard and others, I think I have been able to clarify my views slightly, at least enough to appreciate better what I do not understand. I am enclosing a revision of p. 39 of the original draft of that "Growth" paper, which I had sent you before; you may keep or discard these as you prefer, when you have read them. I think the revision better points up the rather obvious ideas I was trying to get over. On the whole, I thought it would be better to delete any specific reference to your models, until I better understood their application. "Beg the question" in the first version was an unfortunate phrase, perhaps presumptuous as well. Everything that I had in mind by that is expressed in the revision.

I think the root of our trouble is that we are working at very different planes. The propagation, and evolutionary elaboration, of complexity is self-evident, or rather very evident, to a biologist, and we are now concerned with realistic working models of reproduction. It is by now reasonably obvious how one might design some such models on an electromechanical basis; a good chemical analogue to a punch-card reproducer, if we had an equal knowledge of its parts, would take us a long ways towards the experimental initiation of life. But outside living systems, we have not learned how to string autocatalytic ~~stuck~~ molecules together, in an autocatalytic system, in such a way as to simulate a punch card reproduction with more than one or a few bits on it. And none of the chemical machines that we can now devise gives a product of anywhere near the complexity of an organism. I can see that you have been looking for the foundations of an axiomatic theory of reproduction, and that I had been needlessly reading my own mechanical interpretations into it. I would have to ask you what material interpretations are feasible. Without knowing some of the other theorems of your system, I could not begin to say whether they would be helpful in genetic analysis. How might A and A' be understood in biological terms, for example? I can see that written correspondence will be too cumbersome to let us get very far. If I can sustain your interest, could I make an appointment to discuss these matters at closer hand? I will be in Washington at the end of October (28-29) for a panel meeting at the National Science Foundation, and could arrange time on the 27th, or perhaps the 29th. The evening of the 27th would be the most convenient on my part. If you are not too busy, and are disposed to belabor this subject further, I would be indebted to you for the occasion.

Yours sincerely,

Joshua Lederberg
Joshua Lederberg

Professor of Genetics